events and submit their recommendations when the Notice of Proposed Rulemaking is sent to the Commission."

There is no mention of devising "some sort of regulatory inducement . . . to get the Babcock & Wilcox Company to provide the NRC with data on the vessels it has sold." Furthermore, it is neither necessary nor logical within the regulatory process for such actions to be considered.

Marshall's statement regarding the litigation over the reactor at Three Mile Island has no basis. In fact, B & W has worked very closely with the owner of the Three Mile Island reactor, GPU Nuclear, on the PTS issue and has performed a thermal shock evaluation of the Three Mile Island Unit I (TMI-1) vessel. It was concluded in this evaluation that no safety concern exists for the full lifetime of the vessel. GPU Nuclear submitted these results to the NRC in July 1982.

With regard to Marshall's statements about the NRC's not knowing the "exact condition of the B & W vessels," B & W and the utilities with nuclear steam supply systems designed by B & W initiated a Reactor Vessel Materials Program in 1977. Extensive data from this program were submitted to the NRC in March 1981. Other formal submittals were also made to the NRC. In fact, to quote S. H. Hanauer of the NRC from the 1 December NRC meeting on PTS: "B & W has completed a very extensive, perhaps the most extensive review of vessel material properties. . . ."

Finally, three points put the B & W activities related to PTS in proper perspective:

1) B & W, in conjunction with the utilities owning nuclear steam supply systems designed by B & W, led the early PTS investigations and made the first extensive submittals within the industry to the NRC in 1980 and 1981.

2) B & W believes that generic screening criteria are appropriate for determining which plants should ultimately perform plant-specific evaluations to address the PTS issue. Such evaluations have already been submitted to the NRC on the Oconee-1 and TMI-1 plants. Other plant-specific evaluations will be completed as appropriate.

3) On the basis of the data provided by B & W and others, the NRC has calculated values for ranking the operating nuclear plants that are most susceptible to PTS. The list was contained in Enclosure A of "SECY 82-465, NRC staff evaluation of pressurized thermal shock, November 1982" (available at the 9 December NRC meeting). As can be seen from this list, the plants with nuclear steam supply systems designed by B & W have substantial margins and, clearly, are not at the top of the list.

We plan to continue to cooperate with both the NRC and our utility customers to ensure that the PTS issue is responsibly addressed.

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Roy's assertion that the NRC did not discuss means of getting information out of Babcock and Wilcox is incorrect, as may be verified by reading the 9 December transcript of the NRC proceedings (pp. 29, 63, and 67).

It is a matter of record that B & W has been enmeshed in litigation, not on pressurized thermal shock, but on related safety issues arising from the Three Mile Island accident. Many of the company's highest officials, including Roy, gave evidence in a trial that began in November and ended on 20 January.

—ELIOT MARSHALL

Academic Economics Continued

Of the 60 comments on my letter "Academic economics" (9 July, p. 104), all except the two that have been printed in *Science* (8 Oct., p. 108; 10 Dec., p. 1070) express strong, often enthusiastic, support for my criticism of academic economics. None came from the theoretical economists and econometricians whom I challenged.

Jacob Cohen's response (Letters, 10 Dec., p. 1070) itself illustrates what is wrong with the present state of academic economics. He has "no doubt that theory is more glamorous than fact-grubbing" and cites the rational expectations theory as the "hottest [!] theory extant in economics."

According to that theory, government can be shown to be powerless to affect the state of the economy by means of any rationally designed measures of economic policies—the reason for this being the ability of private business to anticipate accurately all rational government moves and assess correctly their potential effects and then to neutralize these effects by means of equally rational (profit-maximizing) counteraction.

A survey similar to that presented in my first letter shows that 57 percent of the 44 papers on the subject of rational expectations published over the last 2 years in seven of the most important U.S. professional journals (*1*) are purely mathematical exercises. Thirty-six percent also contain attempts at empirical implementation of these intricate theoretical constructs. Such attempts involve routine application of elaborate methods of indirect statistical inference applied to a small number of aggregative indices (such as total employment, general price-level, and total gross national product). Only two of the 44 researchers saw fit to engage in the grubby task of ascertaining by means of direct observation how business actually arrives at assessment of future government actions and their potential effects, and whether that assessment was actually rational and correct (as is assumed by proponents of rational expectations theory.)

It is not surprising that, after 20 years of this type of research, "scientific opinion" is still split. Many opponents of government intervention in the operations of the economic system agree with Cohen's characterization of the theory of rational expectations as a "most powerful" and "widely applicable methodological generalization," while their more skeptical colleagues—Robert A. Gordon, for instance—cite (*2*) that theory as an example of a development "in which theory proceeds with impeccable logic from unrealistic assumptions to conclusions that contradict historical record."

Psychologist Robert Glassman's broad philosophical comments (Letters, 8 Oct., p. 108) seem to reflect conditions prevailing in his own discipline. While far from having attained a state of internal cohesion as in the physical sciences, economics certainly can advance beyond the early stage characterized by swings between compulsive empiricism and footloose theoretical speculation. Whatever disagreement exists between proponents of different approaches, the necessity of maintaining a close complementary relationship between construction of theoretical models and their empirical implementation does not seem to be questioned, at least in principle. My strictures are directed against pure theorists and statistical curve-fitters who prefer to leave the grubby fact-finding task to others. Too sharp a division of labor between theoretical and experimental work can lead to mutual misunderstanding, even in so-called exact sciences; in softer disciplines, it is bound to bring about a total impasse.

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References and Notes

1. *Journal of Political Economy* (since October 1980); *American Economic Review* (since September 1980); *Journal of Monetary Economics* (since October 1980); *Econometrica* (since November 1980); *Review of Economics and Statistics* (since November 1980); *Quarterly Journal of Economics* (since November 1980); *Brookings Papers on Economic Activity* (since 1980, No. 1).
2. R. A. Gordon, *Am. Econ. Rev.*, **66** (No. 1), 5 (1976).

Sheep Deaths in Utah

R. Jeffrey Smith's News and Comment article "Scientists implicated in atom test deception" (5 Nov., p. 545) presents a depressing picture of government bureaucrats and unethical scientists whitewashing effects of weapons tests to deceive sheepmen. The bias may only have reflected the judge's ruling in the case described, but one would hope that *Science* might present a more balanced analysis, devoid of scare headlines.

It is a matter of record that the rigors of the winter range have taken heavy tolls of ewes and lambs before and since the era of above-ground testing; this might have been mentioned. The availability of pertinent research data from Hanford Laboratory studies might have been attributed to the Atomic Energy Commission's (AEC's) commendable foresight in sponsoring these studies, rather than to "extraordinary luck." The full disclosure of all results from these studies in reports from the Hanford Laboratory to the AEC might have been commended as proper scientific reporting rather than described by the term, "curiously." And the selection of data for court presentation on the basis of scientifically evaluated relevance might have been defended as a proper exercise of scientific judgment. Had all these things been done, the story would have been less exciting but more in accord with the realities as we (former colleagues of Leo Bustad and Harry Kornberg at Hanford Laboratory) remember them. We were not involved with the sheep studies at Hanford Laboratory, but we will not accept as true any allegations of impropriety, let alone fraud.

In our view, the defendants in the 1956 civil suit brought by sheepmen were guilty of no breach of scientific ethics. They brought to the court their relevant data and their best scientific opinion. Unfortunately, their testimony is not even available now to serve in their defense because transcripts of the 1956 court proceedings were destroyed by the Utah court. If, under such circum-